and believe that in the future their conclusions could radically change and enlarge the role of SLR in the treatment of patients with non–small cell lung cancer.

Our most important concern, however, lies in the cases of unexpected N1 or N2 disease, ranging from 4% to 7% in the experience of Altorki and colleagues[1] and reported at similar levels in the most recent literature. In this group of patients with disease upstaged to stage II or III, SLR is not indicated and LR is certainly the optimal treatment.[3]

In addition, we would like to focus the attention on some features that could limit the meaning of the study. First, their results are based on a population enrolled in the International Early Lung Cancer Action Program (I-ELCAP) group according to specific criteria.[4] This very uniform population may not represent the general population of a daily clinical practice. Second, this was not a randomized study, and the criteria used by the different surgeons to decide whether to perform LR or SLR and the surgical approach adopted are not reported. Third, there were some patients in both groups who did not undergo a complete nodal sampling. This could lead to incorrect staging and influence survival results. Finally, in our experience and according to several studies, wedge resection is associated with a higher rate of recurrence than is segmentectomy. This gap could be determined by several factors, such as smaller parenchymal margin and lower yield of lymph nodes.[5] Also, Altorki and colleagues[1] in their study reported that segmentectomy was associated with a lower recurrence rate than was wedge resection. Despite this, LR and wedge SRL have the same survival. We underline that wedge resection and segmentectomy are not oncologically equivalent and suggest that they be considered separately.

In conclusion, Altorki and colleagues[1] report interesting data, but the topic might be better investigated with a randomized study to draw definitive conclusions.

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RELATIVE AMPLITUDE INDEX: A NEW TOOL FOR HEMODYNAMIC EVALUATION OF PERIPROSTHETIC REGURGITATION AFTER TRANSCATHETER VALVE IMPLANTATION

To the Editor:

Heinz and colleagues[1] present an interesting concept to help clinicians assess the severity of paravalvular leakage (PVL) after transaortic valve insertion; however, their report raises a number of issues.

The association of PVL severity as assessed by echocardiography and poor outcomes after transcatheter aortic valve implantation is well known.[2] It will be difficult to adopt the relative amplitude index (RAI) as a new marker compared with an echocardiographic assessment, because the authors have not presented data on the outcomes for patients with a mild PVL on the echocardiogram and a high RAI versus severe PVL and a low RAI.

With regard to the statistical analysis, no correlation coefficient was presented for PVL severity and RAI. The PVL severity and RAI are almost certainly related; however, no interaction analysis was performed for mortality or long-term survival. The univariate analysis of death did not include age, Agatston score, or annular eccentricity as covariates, probably the most important factors in determining PVL severity.[3] In addition, perioperative respiratory failure had the greatest odds ratio for death, but preoperative chronic obstructive pulmonary disease was not significant, implying that technical issues at implantation could be an issue. No correlation between the RAI and perioperative complications were presented. An RAI cutoff value of 14 was deduced from the receiver operating curve analysis; however, no sensitivity or specificity data were presented. Also, only 7 of the 110 patients in the cohort had an RAI of $\geq$14, suggesting general applicability might be an issue. With regard to mortality, we question their finding of a significant difference ($P = .013$, Table 2), because we calculated the mortality difference as nonsignificant ($P = .1$).

The formula presented for the derivation of RAI was not referenced or

derived in their report, and, despite the simplicity, we believe, from a mathematical and engineering viewpoint, is incorrect, according to previous reports.[4,5]

$$BP_{d\text{-}pre} = F_d \cdot SVR_{pre} \quad (1)$$

$$BP_{s\text{-}pre} = \left(F_s + F_d + F_{r\text{-}pre}\right) \cdot SVR_{pre} \quad (2)$$

$$BP_{d\text{-}post} = F_d \cdot SVR_{post} \quad (3)$$

$$BP_{s\text{-}post} = \left(F_s + F_d + F_{r\text{-}post}\right) \cdot SVR_{post} \quad (4)$$

where BP_s and BP_d is the systolic and diastolic blood pressure, respectively; F_s, F_d, and F_r is the systolic, diastolic, and regurgitant flow, respectively; PP, pulse pressure; SVR, systemic vascular resistance; and subscripts $_{-pre}$ and $_{-post}$ indicate pre- and post-transcatheter aortic valve implantation, respectively.

Subtracting Equation 3 from 4 and 1 from 2, assuming $SVR_{pre} = SVR_{post}$, yields

$$PP_{pre} = F_s \cdot SVR_{pre} \quad (5)$$

$$PP_{post} = (F_s + F_r) \cdot SVR_{post} \quad (6)$$

Dividing Equation 6 by 5 and rearranging yields

$$F_r = \left(PP_{post}/PP_{pre} - 1\right) \cdot F_s \quad (7)$$

Rearranging the formula for RAI in their report would yield a different formula:

$$RAI = \left[\left(BP_d - post/BP_{s\text{-}post}\right) - \left(BP_{d\text{-}pre}/BP_{s\text{-}pre}\right)\right] \cdot 100 \quad (8)$$

However, despite a reasonably large series, multiple logistic regression analysis was not possible owing to issues with a lack of statistical power. Echocardiographic assessment of PVL severity can be difficult owing to interpretation and blood pressure management during general anesthesia. RAI is also dependent on anesthetic management and vasoconstriction administration.

We thank Heinz and colleagues[1] for their work; however, we must caution against the adoption of RAI until these issues have been addressed.

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Reply to the Editor:

We thank Dr Poulis for his kind interest in our article and for his comments. Despite acknowledging the practicability of an index that is based on hemodynamic criteria, he questions the value of the relative amplitude index (RAI) presented in our article. Although the issues raised are of great interest, he has probably misunderstood the main aim of introducing the RAI to assess the impact of paravalvular regurgitation (PAR) after transcatheter valve implantation.

Dr Poulis implies that the RAI has a lower value than echocardiography for assessing PAR. At this point, we would like to take the chance and underline that a comparison of established assessment methods for PAR with RAI was far beyond the aim of the study. It was not our intention to challenge echocardiography for assessment of valve function and PAR postimplantation. We do consider echocardiography to be the criterion standard for anatomic and functional evaluation of the implanted valve, and we regularly use it in all cases for intraprocedural and postprocedural assessment. Although the data depicting the relevance of PAR on survival are unquestionable, little is known regarding the prognostic capacity of detected PAR with respect to outcome. The RAI was therefore designed to distinguish between patients with moderate PAR and positive outcome from those with moderate PAR and negative outcome.[1] Several factors, such as preexisting aortic regurgitation, may influence the impact of PAR on outcome. In this study we were able to show that increased RAI was associated with both a relevant PAR and mortality in the perioperative period and at follow-up. This association is based on the finding that the degree of aortic regurgitation did not correlate with RAI, whereas relevant PAR did. The difference between preoperative and postoperative regurgitation seems to play the major role, however, and the standardized calculation of this difference is the major contribution of RAI.

To elucidate further on the differences in outcome, we would like to mention the following observations: as shown in the Results section, 5 of 7 patients with a RAI of at least 14